ITEE part III Peer Review

By Jolanda Theeuwen

'Impact of biomass burning on pollutants surface concentrations in megacities of the Gulf of Guinea' by Menut et al.

This research is done to quantify the relative contribution of biomass burning in southern and central Africa to the atmospheric composition in mega-cities in the Gulf of Guinea. First the WRF model is used to simulate the meteorological input data for the CHIMERE model in the period of May-July 2014. Second, tracer experiments, releasing tracer at two different heights and two different locations, are done using the CHIMERE model. Third the long-range transport of the pollutants is analysed with three situations: no fires, fire emissions injected in the lower atmosphere and fire emissions injected in the upper atmosphere. From this research can be concluded that first, the biomass burning in this area is contributing to the atmospheric composition in cities in the Gulf of Guinea. Second, transport of the pollutants is different for different emissions heights, however after a couple of weeks the concentrations are similar for the different injections. Third, the biomass burning resulted in a maximum increase of 150, 20 and 4 μgm^{-3} for respectively CO, O_3 and PM_{10} (Particulate Primary matter and Particular Organic matter).

This research is interesting as it is a contribution to the DACCIWA project. The results will add new knowledge on what the influence of the biomass burning is on atmospheric pollution in the area of the Guinea Gulf. The DACCIWA project is set up to analyse the impact of the atmospheric composition on populations health. The project and therefore a contribution to this research, are of social importance because of the highly polluted megacities in western Africa.

This research is a general implication of atmospheric modelling and therefore, I believe it fits the scope of this journal well. Both the physics, the meteorological conditions, and the chemistry, the transport of chemicals, are simulated in this research. These themes fit the subject of the journal well.
The poor use of grammar negatively influences the clarity of writing in this article for me. The article is very long which also has a negative influence on the clarity. However, with the help of summaries in between different sections the article is made clearer for me.
I think the work done in this research is not completely correct. If I would be doing this research I would reconsider the simulated period. Additionally, I would add an uncertainty indication in the results to account for observation and simulation errors. However, I think the overall steps in this research are chosen well and support the conclusion. Because is shows some flaws I recommend the editor not to accept this article yet. However, after some changes I definitely think this article will be a valuable addition to the journal. Some examples of changes are reformulating the introduction; the need and scope need to be introduced better, and adding an uncertainty indication in the conclusion. These examples are explained more thoroughly further on in this peer review.

Strengths
As mentioned before I believe the overall steps in this research are chosen well and support the conclusions. The order of the different parts in this research is correct and each step gives value to the research.
Furthermore, I think that the authors put a great effort in creating structure in this article. In the introduction, the content of the article is clearly explained also the approach of the research is explained shortly. Also the summaries after each section contribute to the overall structure of the article. These summaries allow the reader to put everything into place. The conclusions drawn here are eventually put together in the final conclusion. Throughout the article bullet points are used for summations which also makes the article more readable.

Major points of improvement

The first point of improvement in this research is the choice of the modelling period. The authors reference to Barbosa et al. 1999 (p6 line 28) to explain their choice of the modelling period. The authors state that the fires in central Africa start in April and peak in July and therefore they modelled the months May-July. In the referenced article, I found that fires in Africa on the southern hemisphere indeed start in April and end around October. However, on the northern hemisphere the fire burning activity begins in November. The, in this research investigated, area covers both the northern and the southern hemisphere and therefore I believe that the performed research is incomplete. No specific time period is mentioned in the scope of this research and therefore the goal is to analyse the 'general' contribution of biomass burning emissions. The authors give insight in the atmospheric composition for a period of three months and therefore it does not completely fit the scope of this research I believe. For this research to fit the scope I think that the authors should give insight in fire burning throughout an entire year by modelling also the peak events on the northern hemisphere.

It could be possible that due to meteorological conditions the fires in the northern hemisphere do not influence the pollutant concentrations in the megacities in the studied area. However, if this would be the case the article would be stronger if sufficient evidence would be given. I recommend the authors to look into the atmospheric dynamics in the northern hemisphere by setting up a WRF simulation for this area and use the gained output for additional tracer experiments. This should be done for the months November-January. If the emissions in the northern hemisphere haven an important role in the atmospheric composition of the studied megacities the entire research should be repeated for the months November-January to fit the scope.

Second, in the article is mentioned that the simulated meteorological conditions lead to differences between model and observation, comparing AOD CHIMERE and Modis (p 17 line 10). It is however not discussed what the approximate contribution of this error is to the simulated result. Throughout the entire article this is the only reference to the possible influence on the model simulations by the deviating meteorological input conditions. I think the article will improve if uncertainties are included throughout the article.

Meij et al (2009) shows that different meteorological conditions can have a different influence on the modelled O_3 and PM_{10} concentrations. In this research two simulations are used. One simulation is overestimating the precipitation and the other one is underestimating the precipitation. In Meij et al (2009) the conclusion is drawn that for PM_{10} in January the concentration differs with a factor of 1.6 for the different simulations. This shows that meteorological conditions indeed have a significant influence on the concentrations. According to the reviewed article the change in PM_{10} by biomass burning is $5 \mu g m^{-3}$. I believe an increase or decrease of a factor 1.6 is a significant difference and should therefore be investigated. In the reviewed article the authors state that the meteorological conditions are deviating a bit due to the difficulty of simulating these highly variable processes. However, in the results it is mentioned that some results are quite similar and they are even called 'realistic'. The authors show that both overestimations and underestimations are made in the model. Meij et al (2009) shows that this influences the CHIMERE results.

To improve on this point the author should include the uncertainty more often and more in detail. The author has two options to implement the uncertainty. First, the author can include it by discussing the possible influences of the deviating meteorological conditions on further simulations in more detail. The authors should then discuss for every result the uncertainty of the simulations but also the uncertainty in the observations. These discussions then need to be combined at the final conclusion to give an educated guess of the uncertainty in the research. The second option is to redo the experiments with different settings. An ensemble run can be done to quantify the uncertainty. When the authors perform an ensemble run it will allow them to include error bars in their conclusion on the increase of O_3, CO and PM_{10} concentration. Including these error bars will show that the authors looked

critically at their own results which can be seen as valuable. It will also make the article more credible. Because the conclusion gives a clear quantification of the contribution of biomass burning I personally believe option two fits this research well. However, discussing the insecurities of the performed research is always a good addition to scientific research. This discussion clearly indicates where improvements are needed and where further research is necessary.

The third point of improvement is the content of the introduction and how it fits to the conclusion. In the introduction is stated that this study is done for the DACCIWA project to prepare for a field campaign in June/July 2016. The paper also states that the aim of the DACCIWA project is to define the variability of the atmospheric composition and its impact on West African climate and health. It is however not stated how exactly the results will contribute to this project and the field campaign. I think the content of the current introduction does not clearly describe the need for this research. Also is the aim of the DACCIWA project clearly described but is the aim of this research not strongly present. Without this information, it makes it more difficult to put the results into context. However, this information is important for the reader. This type of information motivates me to keep reading an article. The impact of the paper is also smaller without a clear explanation of the need for this research.

Additionally, the introduction proposes the research question which does not completely fit the conclusions. This research is set up to quantify the relative impact of biomass burning in central Africa on the atmospheric concentrations of O_3, CO and PM_{10} in the Guinea Gulf area. The conclusion does quantify the absolute impact of biomass burning on these concentrations but not the relative contribution. This information could be relevant defining the problem and dealing with the problem. To improve populations health and climate the project should focus on emissions that have a large influence on the pollutant concentration. To know which factors have an important contribution to the atmospheric composition, the total concentrations of O_3, CO and PM_{10} need to be known. To give an indication to the author on how this additional information can be a valuable for the article I looked up some numbers. In Lagos, one of the studied cities, the PM_{10} concentration during the morning is 476.35 μgm^{-3}. During the afternoon, this concertation is 454.60 μgm^{-3} (Odekanle et al. 2016). The increase due to biomass burning is approximately 5 μgm^{-3} this is less than 1%. Concentration of O_3 is approximately 50 ppb and the concentration of CO is between 75 and 300 ppb (Jambert et al. 2017). These concentrations are equal to 100 μgm^{-3} O_3 and 80.88-343.5 μgm^{-3} CO. Respectively the influence of biomass burning on the concentrations is 10-20% and 44-185%. These parts are much larger than the contribution to the PM_{10} concentration. With these percentages, it is clearer what the relative impact of biomass burning is in polluting the atmosphere.

To improve the introduction the author should first include a part that describes the exact contribution of this research to science and also the aim. Additionally, the author should include a part on the average O_3, CO and PM_{10} concentrations in the studied area. This information can then be used in the conclusion to add a part that describes what the relative contribution of biomass burning is to the concentration of these three chemical components. To make the introduction and conclusion fit both need some adjustments.

Minor points of improvement

Minor points concerning figures and tables

I believe figure 2 is not a valuable addition and can therefore be taken out completely.

Data is missing in figures 2 and 13. In section 2 is written: 'Finally, note that, for chemistry, there is a lack of in-situ surface measurements for this region and during the studied period.' This could direct to the chemistry in figure 2. However, I do not understand if the authors indicate the missing data in

figure 13 with this as well. Maybe the authors could specify which observation techniques have missing data instead of saying there is data missing in chemistry.

For the description of figure 5 please include that the observations are presented with dots. The text should include a reason why the observations in this figure are different for 15 May and 15 July.

For figures 6, 7, 10, 12 and 14 there is some overlap between the legend and the graphs.

Please present figure 7 (Lope) and 12 in a cleaner way. The figures now are difficult to read due to different scaling and overlap.

In figure 13 I would put the legend in the image instead of in the description of the figure. I think this will make the figure easier to read and it is more consistent compared to other figures. Additionally, a lot of data is missing in this figure please discuss this.

Table 2 was a bit difficult to read for me. The description is incomplete, it says the table shows the correlation between the observations and the model for de AOD. The description should also include that the observed AOD and the modelled AOD are included in this table.

For table 3 please also include in the description the observed and modelled values for PM_{10} surface concentrations are included in the table. Please also include unities in the first line of the table. Finally, the description says that the RMSE is given in the table but only the bias is presented.

General minor points
The title is very general and therefor does not state the important conclusion. A possibility is: 'The significant impact of biomass burning on pollutants surface concentrations in megacities of the Gulf of Guinea.' If this title is used it supports the conclusion that biomass burning in central Africa must be taken into account when understanding the atmospheric pollution in the Guinea Gulf area.

In the introduction please include a reference on previous research that is similar to this one. Only for every separate part references of previous research are included. I think it will add value when one (or more) reference(s) on modelling fire emissions making use of both WRF and CHIMERE is (are) added. I think it will make the research more reliable because in this article is stated that wild-fire fluxes are in chemistry transport models, one of the most uncertain sources (p6 line 29-30).

The last two paragraphs in the introduction give a clear view on the structure of the research and article. Please include which models are used for each step. I think this will make it easier for the reader to understand already the first time reading the article, which model was used to investigate each step.

IASI CO columns are recovered using the FORLI algorithm. It is unclear to me whether the obtained data is filtered and if so, how is it filtered? Pommier et al. (2017) shows that not all measurements are reliable and the unreliable measurements are filtered out using the RMSE and bias. (p19 line 13-14)

Please add how the vertical levels are ordered in WRF (p 4 line 13).

Minor points in use of grammar
Throughout the entire paper the level of grammar is poor and it disturbs the readability. Some examples are presented below.

- Typos: p 2 line 27-28: groud-based stations --> ground-based stations
- Syntax: p3 line 3: the model validation, the discussion on the biomass burning.. --> the model validation and/or the discussion on the biomass...

- Misuse of word: P 17 line 10 this is possible --> and therefore it is possible
  P23 line 20: Figure 13 and for the.. --> Figure 13 for the...

- Plural: p17 line 21: fires --> fire
  P23 line 18: emissions products --> emission products

- Conjugations: p23 line 12: contains --> contain
- Sloppyness: p 25 line 26: PM10 should be indicated with a count --> and iii) PM10

References

Barbosa, Paulo Marinho, et al. "An assessment of vegetation fire in Africa (1981–1991): Burned areas, burned biomass, and atmospheric emissions." Global Biogeochemical Cycles 13.4 (1999): 933-950.

Jambert, Corinne, et al. "Observations of biogenic isoprene emissions and atmospheric chemistry components at the Savé super site in Benin, West Africa, during the DACCIWA field campaign." EGU General Assembly Conference Abstracts. Vol. 19. 2017.

Meij, A. de, et al. "The impact of MM5 and WRF meteorology over complex terrain on CHIMERE model calculations." Atmospheric Chemistry and Physics 9.17 (2009): 6611-6632.

Odekanle, E. L., et al. "Personal exposures to particulate matter in various modes of transport in Lagos city, Nigeria." Cogent Environmental Science 2.1 (2016): 1260857.

Pommier, Matthieu, Cathy Clerbaux, and Pierre-Francois Coheur. "Determination of enhancement ratios of HCOOH relative to CO in biomass burning plumes by the Infrared Atmospheric Sounding Interferometer (IASI)." Atmospheric Chemistry and Physics 17.18 (2017): 11089-11105.